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and thus a differentiation is set up which may last for even a term of years. These regions of activity may show many changes in its manifestation during that time. The activity may be intermittent; it may oscillate from one latitude to another; it may vibrate backwards and forwards in longitude; and yet its continuity may not be wholly lost. The analogy of the Great Red Spot of *Jupiter* may help us here. It has been continuously under observation for nearly thirty years; very possibly it has existed for more than seven times as long. But taking the shorter period, it has during that time varied much in colour and intensity, slightly in size and shape; it has been very intermittent in its distinctness; its period of rotation has differed much from those of neighbouring spots, and has varied from one year to another. Yet it has preserved its individuality through all, and there is no doubt of the continuity of its existence. Though it is evidently no solid body—for its changes of motion preclude that idea—yet it evidently denotes something of structure, something of specialisation, in that region of *Jupiter*. So though we cannot predicate—or certainly not as yet—any such long-continued specialisation of any regions upon the Sun, yet we think that very strong suggestions have been afforded us that a certain degree of specialisation, though of a less permanent character, is already in progress there.

86 *Tyrwhitt Road, St. John's, Brockley, S.E.*:
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Further Note on Instrumental Errors affecting Observations of the Moon: in reply to Mr. Cowell's Paper of 1904 *June*. By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

Owing to my absence in America, Mr. Cowell's paper of June last did not come to my notice in the ordinary course, and was only read by me comparatively recently. Though I am reluctant to prolong a controversy of little general interest, there are at least two statements in the paper which seem to me to require important corrections, and I hope to indicate them as briefly as possible. The first statement is as follows (bottom of p. 695):

"Professor Turner . . . has thrown aside the observations of what he calls 'day 15' (or full moon) and smoothed out days 14 and 16. These are the most important observations of all, because the parallactic inequality is then most different from its mean value, and we are at once left with an amount of material insufficient for its purpose."

This statement is irrelevant, because I was not dealing with the parallactic inequality or concerned with it in any way whatever. I was discussing *instrumental error*, the motion of the

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Moon with all its characteristics having been eliminated by the simple process of comparing observations made by one instrument with those made by another at virtually the same moment. The observations might have been made, for instance, on a fixed artificial disc. My procedure cannot therefore be criticised from the point of view applicable in the case of the parallactic inequality.

I have pointed this out to Mr. Cowell, but he still maintains that his criticism applies because my discussion of instrumental error suggested a word of caution as to the correctness of his value for the parallactic inequality. The point is rather a logical than a mathematical one, and I will venture to illustrate my contention of irrelevance by an analogy.

Suppose Mr. Cowell to be discussing the chances of a broken leg, to which travellers are liable, from statistics of journeys between London and Liverpool, and that, in order to be strictly just to the railways, he acknowledged that some of the accidents might have occurred at home, setting down, without indicating how he arrived at it, an estimate of that number. Suppose, further, that from the history of stay-at-homes who take no journeys * I arrived at the conclusion that the estimate so made was inadequate; then surely my methods for discussing stay-at-homes are not to be judged by the rules applicable to travellers simply because the conclusion derived from one case happens to have a bearing on the other. To be more specific, suppose I found it advisable to neglect the hours when the stay-at-homes were in bed as not seriously liable to accident (this is taken to correspond to "throwing aside day 15 &c."). Mr. Cowell's attitude seems to me equivalent to stating that "these hours are the most important of all, because *in railway travelling* accidents are most frequent at night." In essentials this seems to me a fair analogy. Mr. Cowell's eyes are fixed so intently on the parallactic inequality that he treats instrumental error as a particular case of it, whereas the form of the latter is quite different and quite arbitrary. It would be as logical to treat a stay-at-home as a particular case of a traveller who made a return journey in no-time, and to insist on his taking out two insurance tickets accordingly.

The second point is with regard to sin D and cos D † on p. 696. Here we have been more fortunate in arriving at an understanding if not an agreement. The question whether a quantity is to change sign at full moon is liable to ambiguity because the longitude of the centre is made up of two terms—

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* My analogy is of course imperfect here, in that had such direct evidence been available Mr. Cowell would naturally have used it. The comparison of two instruments which I made gives no direct evidence of the instrumental error of either; but at least the motion of the Moon (corresponding to the journey) is eliminated, which is the essential point.

† I must apologise for having inadvertently measured D from full moon instead of new moon on p. 568 of vol. lxiv., as Mr. Cowell indicates on p. 696.

$$\underset{\text{longitude of limb}}{P} \pm \underset{\text{observed semidiameter}}{Q}$$

of which P does not, and Q does, automatically change sign at full moon. Errors in longitude of the Moon naturally associate themselves with P, and were chiefly present to Mr. Cowell's mind; instrumental errors, especially the variability in observed semidiameter with approaching daylight, equally naturally associate themselves with Q, and were chiefly present to my mind (as may be seen on reference to *Monthly Notices*, vol. lxiv., bottom of p. 410). Hence it is clear that we cannot decide the applicability or non-applicability of terms such as sin D or cos D by their changing sign at full moon; we merely determine whether they are associated with P or Q.

Thus in the series suggested by me on p. 567

$$a_1 \sin D + b_1 \cos D + a_2 \sin 2D + b_2 \cos 2D + \text{\&c.}$$

for representing instrumental error, if we are thinking of Q we should expect the part

$$C = b_1 \cos D + b_2 \cos 2D + \text{\&c.}$$

to be large and

$$S = a_1 \sin D + a_2 \sin 2D + \text{\&c.}$$

to be small; if we are thinking of P being affected it might be the other way. Hence I do not see that Mr. Cowell's paragraph on p. 696 (when the above possible misunderstanding is cleared away) is any answer to the remark I made on p. 568. For clearness may I recall very briefly the sequence of events?

I raised the question whether the instrumental errors of the old transit circle in use to 1851 were not sensibly different from those of the new one used afterwards.

Mr. Dyson replied (p. 566) that the coefficients of sin D, deduced by Mr. Cowell from the observations, showed no such change.

I rejoined (p. 567) that such evidence was incomplete, since terms involving cos D, cos 2D, &c., had not been included in the series, and that these had probably a very real existence for reasons given.

To this I venture to think no real response has been made either on p. 696 or otherwise. It is true that Mr. Cowell has since looked for, and found, a term $0''\cdot28 \cos D$ (p. 695, line 9) "which must be attributed to errors of observation"; but I believe this is applicable to longitude (called P above) and not to semidiameter (Q above). It seems to me, in the light of our recent discussion, that a complete investigation would contemplate *four* sets of terms: viz. (1) sines applicable to P, (2) sines

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applicable to Q, (3) cosines applicable to P, (4) cosines applicable to Q. But in practice it would be doubtless impossible to separate these terms satisfactorily.

Before leaving the point perhaps I may state my position in other words thus:

Mr. Cowell writes (vol. lxiv. p. 96): "The hypothesis that the error of semidiameter is a constant is the best that I can make."

I venture to doubt this, even in the circumstances considered by Mr. Cowell. I think he should have considered the expression

$$b_0 + b_1 \cos D + b_2 \cos 2D + \&c.$$

instead of the first term only.

The remainder of Mr. Cowell's paper is an admirable exposition of the principles of his analysis, which I am glad to have drawn from him. If I refrain from comment it is because I am afraid of prolonging the discussion unduly. The two points mentioned seem to me important, and I hope I have made my position with regard to them clear. Mr. Cowell and I have, as already mentioned, discussed these matters without being able to arrive at an agreement, so that he will doubtless represent his views clearly also, and there I hope the matter may be left.

Reply to Professor Turner's further Note. By P. H. Cowell.

1. Let me put

N = instrumental error of transit circle (1851–61)
O = „ „ "old transit circle" (1847–50)
L = „ „ altazimuth (1851–61)
M = „ „ „ (1847–50)

Then Professor Turner has shown, by processes that I have no objection to, that N—O—L+M varies from $0''{\cdot}0$ on day 17 to $+1''{\cdot}3$ on day 21, and then decreases to $+0''{\cdot}8$ on day 24, the zero being an arbitrary one (vol. lxiv. p. 411).

Regarding this table merely as proving that the above combination of instrumental error varies with the day of the lunation, it was clearly unnecessary for me to call attention to the absence of days 15 and 16. The importance of these days is, however, considerable when we attempt to consider the law connecting the above observed quantity with the age of the Moon.

I do not wish my chief objections to Professor Turner's first paper to be obscured by a discussion of minor points. What I object to is this. He says L—M may be put equal to zero, and